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FOOD CONSUMPTION AND NUTRITION DIVISION February 2005

FCND Discussion Paper 188

Coping with the "Coffee Crisis" in Central America: The Role of the Nicaraguan *Red de Protección Social*

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Abstract

The international and local Nicaraguan media have widely reported on the "coffee crisis" in Latin America and there is substantial evidence that there has been a downturn and that this has been more severe in the coffee-growing regions. Using household panel data from a randomized community-based intervention carried out in both coffee- and noncoffee-growing areas, I examine the role of a conditional cash transfer program, the *Red de Protección Social* (RPS), during this downturn. While not designed as a traditional safety net program in the sense of reacting or adjusting to crises or shocks, RPS has performed like one, with larger estimated program effects for those who were more severely affected by the downturn. For example, it protected households against declines in per capita expenditures and, while not significantly depressing labor supply relative to before the program, muted additional labor supply for beneficiaries in coffee-growing areas, relative to their counterparts without the program. Beneficiaries who participated in the coffee industry as laborers before the program were more likely to have exited the coffee industry, whereas those who participated as producers were less likely to have exited. The findings are consistent with the existence of credit constraints inhibiting such transitions in the absence of the program. Overall, then, RPS appears to be playing an important part in the risk-coping strategies of households.

Key words: conditional cash transfer program, coffee crisis, social safety net, Nicaragua

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This research builds on the evaluation of the *Red de Protección Social* by the International Food Policy Research Institute (IFPRI). I thank the *Red de Protección Social* team, for their continued support. Natàlia Caldés, Dave Coady, and Laura Rawlings provided many helpful comments, as have seminar participants at IFPRI, the Center for International and Development Education (CIDE), Cornell University, the Northeast Universities Development Consortium (NEUDC), the University of Minnesota, and the World Bank. I gratefully acknowledge funding for this research from the Inter-American Development Bank, through the Norwegian Fund for Social Innovation, and from the World Bank.

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1. Introduction

In spite of some recovery in 1994 and 1997, world coffee prices have been declining since the mid 1980s—in 2002, real prices were at their lowest levels in more than 50 years. The continued downward trend, as well as the recent substantial decrease in prices, has had adverse implications for incomes within many of the coffee-producing countries in Central America. These have been widely reported on in the international and local media as the "coffee crisis."[1] In some cases, prices have reached levels below typical production costs. Though only limited micro-level empirical evidence exists regarding the magnitude and nature of the effects of the price trend, there is a perception that one consequence is that poverty is rising.

In this paper, I explore the effects of the price decline in some of the poorest rural regions of Nicaragua, using household-level panel data collected as part of a randomized evaluation of a conditional cash transfer program. I also examine the role played by the program, *Red de Protección Social* (RPS), in protecting well-being, as well as its effects on labor market supply and activities. To do this, the behavior and outcomes of households who were benefiting from the program to those who were not are contrasted. About half of these households live in coffee-growing areas and many are involved to some extent in the coffee industry. The data are brought to bear on the following questions:

- How have households in coffee-growing areas without the program fared over the period 2000–2002?
- Were households in coffee-growing areas with the program better able to protect household expenditures (particularly on food) and educational and nutritional outcomes than their counterparts in coffee-growing areas without the program? That is, how effective was RPS as a social safety net during the downturn?

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[1] A *New York Times* August 29, 2001, article by David Gonzalez was titled, "A coffee crisis' devastating domino effect in Nicaragua."

- Were labor supply and the mix of coffee, noncoffee agricultural, and nonagricultural activities within the household different among households in coffee-growing areas with and without the program? That is, did RPS enable different labor responses to the downturn?

Essentially, I explore whether, and how, the program enabled alternative responses to the downturn. While much of the emphasis in the paper is on the so-called coffee crisis, the results have broader implications in that they demonstrate how safety net programs like RPS condition behavior during an economic downturn.

The findings indicate that, while not originally designed as a traditional safety net program in the sense of reacting or adjusting to crises or shocks, RPS has performed like one, with larger estimated program effects for those who were more severely affected by the downturn. For example, it protected households against declines in per capita expenditures and, while not significantly depressing labor supply relative to before the program, muted additional labor supply for beneficiaries in coffee-growing areas, relative to their counterparts without the program. Beneficiaries who participated in the coffee industry as laborers before the program were more likely to have exited the coffee industry, whereas those who participated as producers were less likely to have exited. The findings are consistent with the existence of credit constraints inhibiting such transitions in the absence of the program. Overall, RPS appears to be playing an important part in the risk-coping strategies of households.

2. Design and Implementation of the *Red de Protección Social*[2]

Modeled after the Programa Nacional de Educacion, Salud y Alimentacion (PROGRESA) in Mexico (Morley and Coady 2003), RPS is designed to address both current and future poverty via cash transfers targeted to households living in extreme

[2] This section draws from Maluccio and Flores (2004), which provides a more complete description of the program.

poverty in rural Nicaragua. The transfers are conditional, and households are monitored to ensure that children are, among other things, attending school and making visits to preventive health-care providers. When households fail to fulfill those obligations, they lose their eligibility. By targeting the transfers to poor households, the program alleviates short-term poverty. By linking the transfers to investments in human capital, the program addresses long-run poverty. RPS's specific objectives include

- supplementing household income for up to three years to increase expenditures on food,
- reducing school desertion during the first four years of primary school, and
- increasing the health-care and nutritional status of children under age 5.

To permit an assessment of how a complex program like RPS has altered behavior of households during an economic downturn, it is first necessary to describe the program's operation and evolution.

Program Targeting

In the design phase of RPS, rural areas in all 17 departments of Nicaragua were eligible for the program. The focus on rural areas reflects the distribution of poverty in Nicaragua—of the 48 percent of Nicaraguans designated as poor in 1998, 75 percent resided in rural areas. For the pilot, the Government of Nicaragua (GON) selected the departments of Madriz and Matagalpa from the northern part of the Central Region, on the basis of poverty as well as on their capacity to implement the program. This region was the only one that showed worsening poverty between 1998 and 2001, a period during which both urban and rural poverty rates declined nationally, and this downturn has been attributed in part to the decline in coffee prices (World Bank 2003). In 1998, approximately 80 percent of the rural population of Madriz and Matagalpa was poor and half of those, extremely poor (IFPRI 2002). Coffee is grown in many parts of Matagalpa, which lies at an altitude appropriate for its cultivation (above 800 meters). In addition, these departments had easy physical access and communication (including being less than a one-day drive from the capital, Managua, where RPS is headquartered), relatively strong institutional capacity and local coordination, and reasonably good coverage of health posts and schools (Arcia 1999).

In the next stage of geographic targeting, 6 (out of 20) municipalities were chosen based on criteria similar to those used at the department level. The 6 were well targeted in terms of poverty. Between 36 and 61 percent of the rural population in each of the chosen municipalities were extremely poor and between 78 and 90 percent were extremely poor or poor (IFPRI 2002), compared with national averages of 21 and 45 percent, respectively (World Bank 2003). While not the poorest municipalities in the country (or in the chosen departments for that matter), the proportion of impoverished people living in these areas was still well above the national average.

In the last stage of geographic targeting, a marginality index based on information from the 1995 National Population and Housing Census was constructed, and an index score calculated for all 59 rural census *comarcas*[3] in the selected municipalities. The index was a weighted average of a set of poverty indicators (including family size, access to potable water, access to latrines, and illiteracy rates) in which higher index scores were associated with more impoverished areas (Arcia 1999).[4] The 42 *comarcas* with the highest scores were eligible for the pilot phase's first stage and were included in the evaluation.

Program Design

RPS has two core components: food security, health, and nutrition; and education.

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[3] Census *comarcas* are administrative areas within municipalities that include between one and five small communities, each averaging 100 households.

[4] IFPRI (2002) describes the RPS targeting in more detail.

Food Security, Health, and Nutrition

Each eligible household received a bimonthly (every two months) cash transfer known as the “food security transfer,” contingent upon attendance at bimonthly health educational workshops held within the community and on bringing their children under age 5 for scheduled preventive health-care appointments. The specific health-care services required by the program were provided free of charge to beneficiary households, and included growth monitoring, vaccination, and provision of antiparasites, vitamins, and iron supplements. Children under age 2 were seen monthly and those between 2 and 5, bimonthly.

Education

Each eligible household received a bimonthly cash transfer known as the “school attendance transfer,” contingent on enrolment and regular school attendance of children ages 7–13 who had not completed fourth grade. Additionally, for each eligible child, the household received an annual cash transfer intended for school supplies (including uniforms and shoes) known as the “school supplies transfer,” and contingent on enrolment only. Unlike the school attendance transfer, which was a fixed amount per household regardless of the number of children in school, the school supplies transfer was a per-child transfer. To provide incentives to the teachers, who had some additional reporting duties and were likely to have larger classes after the introduction of RPS, and to increase resources available to the schools, there was also a small cash transfer, known as the “teacher transfer.” Delivery of these funds to the teacher was monitored (and was a program condition), though not their ultimate use.

Table 1 summarizes the eligibility requirements and demand- and supply-side benefits of RPS. Nearly all (95 percent) of the households were eligible for the food security transfer, and this cash transfer was a fixed amount per household, regardless of household size. Households with children ages 7–13 who had not yet completed the fourth grade of primary school were also eligible for the education component of the program.

Table 1—Nicaraguan RPS eligibility and benefits in the pilot phase

	Program components	
	Food Security, Health, and Nutrition	**Education**
Eligibility Geographic targeting	All households	All households with children ages 7–13 who have not yet completed fourth grade of primary school
Demand-side benefits Monetary transfers	**Food security transfer** C$2,880 per household per year ($224)	**School attendance transfer** C$1,440 per household per year ($112) **School supplies transfer** C$275 per child beginning of school year ($21)
Supply-side benefits Services provided and monetary transfers	**Bimonthly health education workshops** Child growth and monitoring Monthly (0–2 year olds) Bimonthly (2–5 year olds) Provision of antiparasites, vitamins, and iron supplements Vaccinations (0–5 year olds)	**Teacher transfer** C$60 per child per year given to teacher/school ($5)

The amounts for each transfer were initially determined in U.S. dollars and then converted into Nicaraguan córdobas (C$) in September 2000, just before RPS began distribution. Table 1 shows the original U.S. dollar annual amounts and their cordoba equivalents (using an exchange rate of C$12.85 to US$1): the food security transfer was $224 a year and the school attendance transfer, $112. On its own, the potential food security transfer represents about 13 percent of total annual household expenditures in beneficiary households before the program. A household with one child benefiting from the education component would receive additional transfers of about 8 percent, yielding a total potential transfer of approximately 21 percent of total annual household expenditures. Over the two years, the actual average monetary transfer (excluding the teacher transfer) was approximately $300 (or 18 percent of total annual household expenditures). The value of the supply-side services, as measured by how much RPS paid to the providers, was also substantial. On an annual basis, the education workshops cost approximately $50 per beneficiary and the health services for children under age 5, approximately $110.

To enforce compliance with program requirements, beneficiaries did not receive the food and/or education component(s) of the transfer if they failed to carry out any of the conditions described above. During the first two years of delivering transfers, approximately 10 percent of beneficiaries were penalized at least once and therefore did not receive one or both of the transfers. Only the designated household representative could collect the cash transfers, and where possible, RPS appointed the mother to this role. As a result, more than 95 percent of the household representatives were women. These representatives attended the health education workshops and were responsible for ensuring that the requirements for their households were fulfilled.

Program Impact

Before examining the role of RPS during an economic downturn, I summarize the findings from the evaluation. Overall, RPS had positive and significant double-difference estimated average effects on a broad range of indicators and outcomes. Where it did not, it was often due to similar, though smaller, improvements in the control group. Nearly all estimated effects were larger for the extremely poor, often reflecting their lower starting points (e.g., percentage of children enrolling before the program). Among poorer beneficiaries, there was simply more potential for improvement on many of the indicators. As a result, the program has reduced inequality of these outcomes across expenditure classes.

RPS in its pilot phase supplemented per capita annual total household expenditures by 18 percent, on average. For beneficiary households, this increase compensated for the large income loss experienced by nonbeneficiaries during this period, and produced a small overall increase in expenditures. Most of the increase in expenditures was spent on food; the program resulted in an average increase of $C566 in per capita annual food expenditures and an improvement in the diet of beneficiary households. Expenditures on education also increased significantly, though there was no discernable effect on other types of investment expenditures. Labor market participation was apparently little changed with the program, though there was an indication of slightly fewer hours worked, on average, in the last week. The economic difficulties experienced by these communities enabled RPS to operate somewhat like a traditional social safety net, aiding households during a downturn.

For schooling, RPS produced a massive average net increase in enrolment of 18 percentage points and an even larger increase (23 percentage points) in current attendance for the target population, whose initial enrolment and attendance rates were 70 and 62 percent, respectively. Examining the number of children in Grades 1–4 who advanced two grades between 2000 and 2002, RPS led to an average increase of 7 percentage points, despite the fact that advancement past fourth grade was not a formal requirement of the program. In tandem with the increased schooling, the percentage of children ages 7–13 that were working declined from 17 to 12 percent.

RPS also induced an average net increase of 11 percentage points (over an initial 70 percent) the percent of children under 3 years of age who were attending preventive health controls. At the same time, the services provided by the program, as measured by process indicators including whether the child was weighed and whether their health card was updated, improved to an even greater extent. Participation by children ages 3–5 also increased substantially. While not possible to statistically demonstrate that RPS increased vaccination coverage for children ages 12–23 months in the intervention group (relative to the control group), it was demonstrated that vaccination rates climbed over 30 percentage points to above 90 percent coverage in the intervention and control areas at a time when they were, on average, decreasing in the remaining *comarcas* in the very same municipalities. One would be hard pressed not to attribute at least some part of this substantial improvement to RPS.

Finally, the more varied household diet and increased use of preventive health-care services for children have been accompanied by an improvement in the nutritional status of beneficiary children age 5. The net effect was a 5-percentage point decline in the percentage of children who were stunted, which still remains high at 37 percent. This decline is more than one-and-a-half times faster than the rate of annual improvement seen at the national level between 1998 and 2001—very few programs in the world have been able to show rigorously such a decrease in stunting in such a short time. Despite improvements in the distribution of iron supplements to these same children, however, RPS was unable to improve hemoglobin levels or to lower rates of anemia.

3. Data Sources, the Setting, and Methodology

Data Sources

The evaluation design was based on a randomized, community-based intervention with measurements before and after the intervention in both intervention and control *comarcas*. One-half of the 42 *comarcas* were randomly selected into the program; thus, there are 21 *comarcas* in the intervention group and 21 distinct *comarcas* in the control group (IFPRI 2001). Given the geography of the program area, control and intervention *comarcas* are in some cases adjacent to one another, a concern considered below. The data used here are from an annual household panel data survey implemented in both intervention and control areas of RPS before the start of the program in 2000 and in 2001 and 2002, after the program began operations. The questionnaire was a comprehensive household questionnaire based on the 1998 Nicaraguan Living Standards Measurement Survey (LSMS) instrument (Grosh and Glewwe 2000), expanded in some areas (e.g., child health and education) to ensure that all the program indicators were captured, but cut in other areas (e.g., income from labor and other sources) to minimize respondent burden and ensure collection of high quality data from a single interview. As a result, one area where it is weaker than the typical LSMS comprehensive household survey is the employment module; the RPS survey only covers activities carried out in the last week and does not ask about earnings from those or any other activities. An anthropometric module for children under age 5 was also implemented in 2000 and 2002, but not in 2001. A *comarca*-level community survey was implemented in 2001.

The survey sample is a stratified random-sample at the *comarca* level from all 42 *comarcas* described above. Forty-two households were randomly selected from each *comarca* using as the sample frame a census carried out by RPS three months prior to the survey, and yielding an initial target sample of 1,764 households.[5] The first wave of fieldwork was carried out in late August and early September 2000, without replacement—that is, when it was not possible to interview a selected household, another household was not substituted. As such, when appropriately weighted, the sample is statistically representative of these 42 *comarcas*, and comprises a relatively poor part of the Central Region in Nicaragua. It is not statistically representative of the six municipalities as a whole (or other areas of Nicaragua, for that matter).

While there was a great deal of progress in getting RPS started throughout 2001, it was not possible to design and implement all the components on schedule. In particular, the health-care component of the intervention was not initiated until June 2001. There were also delays in the payment of some transfers to households during the year, because a governmental audit (not due to the program) effectively froze RPS funds. As a result, the RPS 2001 follow-up survey was delayed until the beginning of October, to allow additional time for the interventions to take effect and for five of the scheduled six payments to be effected. Of course, the advantage of the original plan, with the scheduled RPS 2001 follow-up at exactly the same time of year as in the 2000 baseline, was that it would enable us to control better for seasonal variation, for example, in expenditures or labor force participation. With a control group, however, the possible bias introduced by seasonality can be controlled for statistically. This difference in the timing of the survey, then, does not present a serious problem for the estimation of average program effects, though it is a potential problem for making definitive statements

[5] IFPRI (2001) describes the sample size calculations and Maluccio and Flores (2004) describe the baseline and follow-up samples in more detail.

about changes over time *within* the control group, a concern addressed in Section 4. The 2002 survey was also carried out in October, and in the second year, beneficiaries received all components of the program for a full 12 months.

As always in any panel survey, first round non-response and attrition in the survey are potential concerns for the analysis. Overall, 90 percent (1,581) of the random sample of households was interviewed in the first round (see Table 2). In a handful of *comarcas*, the coverage was 100 percent, but in six, it was fewer than 80 percent. For the follow-up surveys in 2001 and 2002, the target sample was limited to these 1,581 first-round interviews. In 2002, just over 90 percent of these were interviewed, on a par with similar surveys in other developing countries (Thomas, Frankenberg, and Smith 2001; Alderman et al. 2001). Again, however, coverage in six of the *comarcas* was substantially worse, where less than 80 percent were successfully reinterviewed. This attrition is unlikely to have been random, a theme taken up in Section 4. Because the same target sample was used in 2002 as in 2001, regardless of whether the household was interviewed in 2001, some households that *were not* interviewed in 2001 *were* interviewed in 2002, and vice versa. The sample of households for which there is a complete set of observations (one

Table 2—Nicaraguan RPS evaluation survey non-response and subsequent attrition

	2000	2001	2002
Completed interview	1,581	1,490	1,434
	(89.6)	(94.2)	(90.7)
Completed interview in all three rounds	1,396	1,396	1,396
	(79.1)	(88.3)	(88.3)
of which			
Intervention	706	706	706
(percent of targeted intervention sample)	(80.0)	(87.2)	(87.2)
Control	690	690	690
(percent of targeted control sample)	(78.2)	(89.5)	(89.5)
not interviewed			
Uninhabited dwelling	60	51	83
Temporary absence	100	28	46
Refusal	16	6	12
Urban (misclassified as rural)	6	0	0
Lost questionnaire	0	6	6
Target sample	1,764	1,581	1,581

Note: Percent of target sample in parentheses.

in each of the three survey rounds) is 1,396, smaller than the 1,434 shown in the first row of the third column of Table 2. The households are about evenly divided between intervention and control groups, indicating that the level of attrition, at least, was not significantly different between them.

The Importance of Coffee in Nicaragua and the "Coffee Crisis"

Coffee production in Nicaragua more than doubled from 932 thousand quintals (or hundred-weight) in 1990 to 2,083 in 2000, but declined to 1,800 in 2001.[6] Over this 10-year period, productivity increased dramatically, with on-farm average yields more than doubling. The vast majority of coffee produced in Nicaragua is exported, and most of it is strictly high grown (SHG) arabica (grown at altitudes greater than 800 meters) and therefore commands a high price; indeed, Nicaraguan coffee often sells at a premium.[7] Over the last five years, coffee exports averaged $140 million, or about one-quarter of total export earnings, and it was the single most important agricultural export (Kruger, Mason, and Vakis 2004).

In addition, the coffee sector is a major employer in the rural economy. Estimates of the importance of coffee in rural labor markets vary widely, with the most reliable based on the Nicaraguan LSMS, which indicates that 20 percent of the rural labor force were employed at some point in the year in the coffee sector (Kruger, Mason, and Vakis 2004). Clearly, coffee is an important source of rural employment. Approximately two-thirds of this employment is seasonal, while the remainder is self-employed or permanent farmworkers (Varangis et al. 2003).

Hence, despite the fact that Nicaragua is only a minor producer on the world stage—and therefore a price-taker in world markets—coffee is a major export crop and employer for the Nicaraguan economy, and declining prices have had important effects on the economy. International nominal year-end prices in U.S. cents per pound reported

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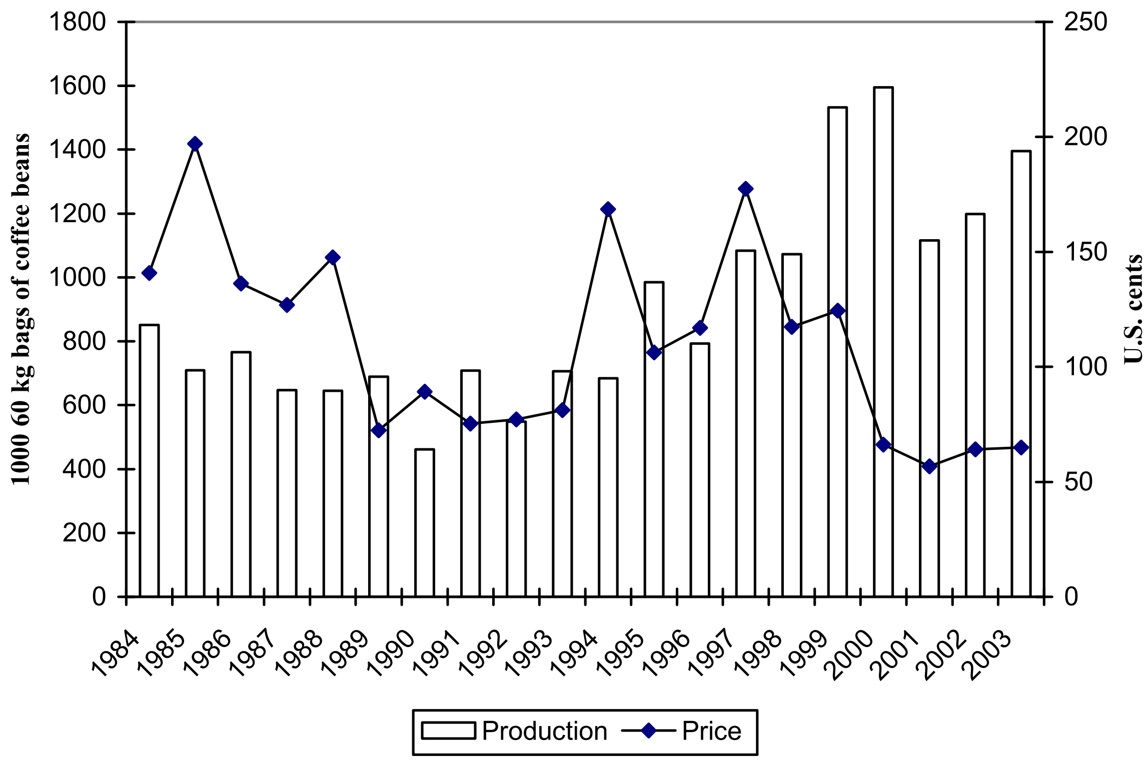
[6] Except where otherwise cited, statistics cited in this paragraph are drawn from Varangis et al. (2003).

[7] For example, in July 2002, it sold for a $3 premium over the New York coffee C contract price for September delivery at the exchange for SHG arabica per quintal.

for "other mild arabica" coffee (the group relevant for Nicaragua) were nearly 180 in 1997 but had dropped over 50 percent to 66 cents in 2000, after which it fluctuated between 55 and 65 cents to the end of 2003 (Figure 1). Unit export values declined in recent years in tandem with the price declines, from $121 per quintal in 1997 to $81 in 2000, and $54 in 2001. The latter prices are unlikely even to cover production costs for some producers (Lewin and Giovannucci 2003). It is this fact that, while perhaps not coming as a surprise (to coffee analysts, anyway), leads many to refer to the current situation as a crisis. Many farmers have been forced to reduce or abandon coffee production, and it has been estimated that 35,000 permanent and 100,000 seasonal jobs were lost (IDB 2001).

Figure 1—December average international coffee prices (Other Mild Arabica Group) and Nicaraguan production in 1,000 60-kilogram bags, 1984–2003



Source: International Coffee Organization.

Coffee Cultivation in the RPS Sample

Via a *comarca*-level survey that accompanied the household-level instrument in 2001 and was administered to key informants, 21 of the 42 *comarcas* in the sample were identified as being areas where coffee is cultivated, 10 in the intervention group and 11 in the control group (see Table 3). Because the *comarcas* are spread across six municipalities in two departments, however, this apparently even allocation masks the fact that all the coffee-producing areas are located in the department of Matagalpa, which is about 100 kilometers closer to Managua than Madriz. As such, in addition to analyzing the complete sample, Section 4 also assesses whether, and how, limiting the sample to *comarcas* in Matagalpa changes any of the results presented below.

Table 3—Coffee cultivation in RPS sample at *comarca* level

Type of *comarca*	Coffee cultivating	Noncoffee cultivating	Total
Intervention	10	11	21
Control	11	10	21
Total	21	21	21

From the labor force participation questions asked in each survey about the previous week, one can glean partial information on the extent to which individuals and households are participating in the coffee industry. For agricultural labor activities, the type of crop was not numerically coded. Interviewers, however, were required to write a brief description of the activity and when coffee was involved, the description typically included the word "café." All jobs in which "café" was noted down are treated as coffee-sector jobs—therefore figures presented below most likely represent a lower bound for individual and (to a lesser extent) household-level participation in the last week, since it seems likely that interviewers at times neglected to specify coffee when the work was in coffee. In addition, given the seasonal and sporadic nature of coffee production, the one-week reference period is almost certainly inadequate to capture all those who ever work in coffee, and is also very likely to miss many of those who occasionally work in coffee (for example, only during the harvest season), further understating involvement in the industry, though it is difficult to say by how much, using only the RPS evaluation data. Rather than focus on levels, then, the emphasis is placed on changes in participation over time. Nevertheless, in describing patterns and descriptive regressions using this information, I underline that in comparison with the other analyses, results that pertain to household-level participation in the coffee sector are less definitive.

While in August and September 2000 nearly 8 percent of those reporting that they had worked in the previous week indicated that they worked in coffee, this percentage had dipped to under 5 percent in 2001 and 2002. These workers were spread across 14 percent of the households in 2000 and 10 percent in 2001 and 2002. These percentages appear to be low in comparison with estimated levels of about 20 percent from the nationally representative rural subsample of the 2001 LSMS, likely due to underreporting for the reasons discussed earlier. Hardly any of the coffee workers resided outside an identified coffee growing area, however, so the percentage participating in those areas alone is twice as large. This pattern is consistent with the demarcation of coffee and noncoffee regions and suggests that the *comarca*-level information is broadly accurate. As with most crops, the demand for casual labor in coffee rises during the harvest season, which can begin in October, but peaks in December and January. The decline between 2000 and 2001/2, then, is somewhat surprising, since during a typical year the seasonal difference in the timing of the survey would lead to more reported coffee work in the October period, not less. This is the first piece of evidence suggesting that participation in the coffee sector is declining.

Between 10 and 15 percent of those reporting working in the coffee sector indicated that they were self-employed farmers (from less than 2 percent of all households), and this percentage changed little over the three surveys. Over two-thirds of those working in coffee are men and only 10 percent are children. In 2000, 7 percent of those working in coffee indicated that they were employed as permanent workers on a coffee farm, but virtually none did in 2001 or 2002, consistent with local media reports that larger coffee farms (which are the ones that employ permanent laborers) had to release labor in recent years. As a result of these small sample sizes, it is not feasible to distinguish between coffee farmers (the sample has, on average, 30 in each year) and laborers in most of the analyses presented below. A simple comparison of per capita expenditures across these two groups in the coffee sector, however, does show that coffee farmers were substantially better off in 2000, with 30 percent higher average expenditures than households with coffee laborers who were not self-employed.

The average percentages across the years masks the fact that many individuals and households report moving in and out of coffee—only one-third of the households reporting participation in coffee in 2000 also report participation in 2002, for example. This movement is shown in household-level transition matrices between 2000 and 2001 (Table 4a) and between 2001 and 2002 (Table 4b). A household is defined to be in the coffee sector if any adult (aged 15 or older) in the household reported any sort of participation in coffee in the last week. Between 2000 and 2001, there seems to have been significant exit from the coffee sector (and this despite the timing of the survey, which favors greater participation in October than in August and September)[8] whereas, on net, only 1 percent exited between 2001 and 2002. Much of the variability or churning (e.g., in 2001–2002, where nearly as many households entered as exited) is almost certainly due to the short reference period considered and likely does not reflect longer-run changes. Thus the patterns seen here are consistent with the media representation of a crisis in the 2000–2001 season, and consequent reduced labor demand on coffee farms; households appear to have been adjusting and "exiting" coffee over time in these areas. Of course, this description of the data considers the entire sample and therefore conflates effects of the crisis with those of RPS. Similar, though slightly weaker patterns emerge when the analysis is restricted to households in the control group only. Transitions into and out of coffee are analyzed more formally in Section 4.

[8] Supporting the hypothesis that there is seasonal variation in participation in coffee is evidence taken from the May 2000 RPS population census, which shows only 4 percent of those reporting working in coffee in the last week, since coffee labor demand is even lower during that part of the year.

Table 4a—Coffee cultivation in RPS sample at household level (2000–2001)

	Coffee cultivating in 2001	Noncoffee cultivating in 2001	Total
Coffee cultivating in 2000	74 (5.3)	128 (9.2)	202 (14.5)
Noncoffee cultivating in 2000	65 (4.7)	1,129 (80.8)	1,194 (85.5)
Total	139 (10.0)	1,257 (90.0)	1,396 (100.0)

Table 4b—Coffee cultivation in RPS sample at household level (2001–2002)

	Coffee cultivating in 2002	Noncoffee cultivating in 2002	Total
Coffee cultivating in 2001	59 (4.2)	80 (5.7)	139 (10.0)
Noncoffee cultivating in 2001	72 (5.2)	1,185 (84.9)	1,257 (90.0)
Total	131 (9.4)	1,265 (90.6)	1,396 (100.0)

Econometric Methodology

The empirical approach exploits two key features of the data allowing one to overcome the vast majority of concerns regarding econometric estimation and causal inference: (1) the randomized design of the evaluation and (2) the panel structure, i.e., the fact that the same households were interviewed over time, before and after RPS was implemented and in both intervention and control *comarcas*. I estimate a series of reduced form specifications that essentially estimate program effects, differentiating them for households residing in coffee or noncoffee growing areas.

The methodology used is based on difference-in-difference techniques and yields what is commonly referred to as the "average program impact."[9] The resulting measures can be interpreted as the expected effect of implementing the program in a similar population elsewhere. The method is shown in Table 5. The columns distinguish

[9] Ravallion (2001) provides a useful and enjoyable discussion of this and related evaluation tools. Maluccio and Flores (2004) provide an additional interpretation for the double-difference estimator.

between groups with and without the program (denoted by I for intervention and C for control) and the rows distinguish between before and after the program (denoted by subscripts 0 and 1). Anticipating one of the analyses presented below, consider the measurement of school enrolment rates for children. Before the program, one would expect the average percentage enrolled to be similar for the two groups, so that the quantity $(I_0 - C_0)$ would be close to zero. After the program has been implemented, however, one would expect differences between the groups as a result of the program. Furthermore, because of the random assignment, we expect the difference $(I_1 - C_1)$ to measure the effect directly attributable to the program. Indeed, $(I_1 - C_1)$ is a valid measure of the average program impact under this experimental design. A more robust measure of the effect, however, would account for any preexisting observable or unobservable differences between the two randomly assigned groups: this is the double difference obtained by subtracting the preexisting differences between the groups, $(I_0 - C_0)$, from the difference after the program has been implemented, $(I_1 - C_1)$.

Table 5—Calculation of the double-difference estimate of average program effect

Measurement	Intervention group with RPS program	Control group without RPS program	Difference across groups
Follow up	I_1	C_1	$I_1 - C_1$
Baseline	I_0	C_0	$I_0 - C_0$
Difference across time	$I_1 - I_0$	$C_1 - C_0$	Double-difference $(I_1 - C_1) - (I_0 - C_0)$

For this work, the double-difference technique is extended to consider the differential effect of the program depending on whether or not the household resides in a *comarca* where coffee is cultivated. The estimating equation for two periods is shown in equation (1).

$$E_{ict} = \alpha_0 + \alpha_1 Y_t + \alpha_2 K_c + \alpha_3 P_c + \alpha_4 Y_t K_c + \alpha_5 K_c P_c + \delta_2 Y_t P_c + \delta_3 Y_t P_c K_c + (\mu_c + \mu_i + \nu_{ict}), \qquad (1)$$

where

E_{ict} = outcome variable of interest for household i in *comarca* c at time t;

Y_t = (1) if second period (or year);

K_c = (1) if coffee cultivating *comarca* c;

P_c = (1) if program *comarca* c;

μ_c = all (observed and unobserved) *comarca*-level time invariant factors;

μ_i = all (observed and unobserved) household-level time invariant factors;

ν_{ict} = unobserved idiosyncratic household and time varying error; and

all the α_j and δ_j are unknown parameters.

The parameters of interest are δ_2, the "double difference" estimator of the average program effect in noncoffee-growing areas and δ_3, the estimator of the differential average program effect in coffee-growing areas relative to noncoffee-growing areas. The total estimated program effect in coffee-growing areas, then, is $\delta_2 + \delta_3$. Because the specification does not condition on household participation in the program, but only on whether the household resides in a *comarca* that has the program, the estimates reflect the "intent-to-treat" effect of the program (Burtless 1995).

One potential concern about the classification into coffee-growing areas is that because coffee cultivation requires specific agroclimatic conditions, the opportunity set for production technologies may differ across areas that do and do not cultivate coffee. Put another way, the fact that coffee is grown in an area is related to other production and labor market decisions in the area. A second concern is that households choosing to live in coffee- or noncoffee-cultivating regions are different in other ways that may be directly associated with the outcome variables under consideration. These suggest possible correlation between the coffee-region indicator and μ_c or μ_i, that, if not controlled for, would contaminate estimates of all coefficients on any variable including the coffee indicator, with the important exception of variables interacted with the random program dummy variable, P_c. To avoid this possibility, household fixed effects are controlled for in all but two of the analyses, thereby controlling for any time-invariant unobserved heterogeneity that may be associated with the location of the household. Another implication of including household fixed effects is that all estimated results implicitly include *comarca*-level fixed effects as well, so that the potential problem that the coffee indicator is correlated with any omitted fixed factors at the *comarca* level is controlled for. Of course, all other time-invariant factors, such as K_c, now drop out of the relation and (extending to all three survey years) we are left with the main estimating equation (2) with Y_1 (Y_2) a dummy for 2001 (2002).

$$E_{ict} = \alpha_0 + \alpha_1 Y_1 + \alpha_2 Y_2 + \alpha_3 Y_1 K_c + \alpha_4 Y_2 K_c + \delta_{21} Y_1 P_c + \delta_{22} Y_2 P_c + \delta_{31} Y_1 P_c K_c + \delta_{32} Y_2 P_c K_c + (\mu_i + \mu_c + \nu_{ict}), \qquad (2)$$

where δ_{21} is the "double difference" estimator for 2001 (relative to 2000) and δ_{22} for 2002 (relative to 2000). δ_{31} and δ_{32} are the respective estimators of the differential effect on households residing in coffee-growing areas.

The coffee versus noncoffee classification at the *comarca* level is necessarily crude—neither the coffee labor nor the coffee production markets are completely segregated across *comarcas* in the sample. The fact that coffee is cultivated in an area does not imply that all households in the area participate in the coffee industry (via labor, production, marketing, etc.) and those not directly participating could be affected less by coffee price declines though they could still be affected, for example, by changes in labor demand and supply for noncoffee-sector jobs. Conversely, households living in areas where coffee is not planted may still participate in the industry as (temporary) migrant laborers, a common practice during harvest periods, so that they could be directly affected by price declines. As a result, the estimated coefficient on an indicator of whether an area has coffee cultivation (relative to one that does not) interacted with the year 2001, for example, will tend to understate the size of the effect of the price decline for those households *actually* participating in the industry, and therefore directly affected. The approach has the advantage of being conservative, thereby increasing confidence in the results when significant differences are found across coffee- and noncoffee-growing areas.

Finally, I emphasize that program effects are identified by the randomized design of the intervention. They do not, for example, condition on household choices or rely on treating the rapid coffee price decline as a shock—an assumption that is hard to maintain in the face of historical trends and given that the recent downturn in prices began in the late 1990s, whereas the data examined here start in 2000.[10] As such, however, the results presented below are likely to understate the effects of RPS in coffee-growing regions to the extent that households have already undertaken various strategies in reaction to the continued price declines before the survey work began in late 2000. This is in the same direction as the possible biases described above and reinforces the claim that the methodology used is a conservative one. I emphasize throughout that the estimates presented refer to the effects of the program during an economic downturn, and not in response to an economic shock.

In the analysis that follows, I work with all (relevant) individuals or households from the balanced panel sample (of 1,396 households interviewed three times each) to keep from changing sample composition in estimating the differences between 2000 and 2001 and between 2000 and 2002. In all but the few instances that are indicated, the estimates control for household-level fixed effects as described above.

4. Results

In this section, evidence addressing the three questions posed in the introduction is presented. To do so, outcome measures of well-being at the household level (per

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[10] While coffee prices have declined substantially in recent years, the current crisis was not unexpected since the industry has been anticipating large increases in Brazilian output for some time. "Apart from over-supply, there are two principal factors underling the current crisis:

- a structural change in the nature of supply, particularly increases in both the quantity and quality of Brazil and Vietnamese coffees, and
- structural changes in demand, comprising increasing demand for high-end specialty products, new technology allowing greater flexibility in blending, and generational shifts in the appeal of different types of coffee products" (Lewin and Giovannucci 2003, 5).

capita total and per capita food expenditures) and indicators of labor supply and activity mix are examined. On finding that there is a substantial downturn in control areas during the 2000–2002 period, I consider the extent to which it has reached down to children in the household, particularly in terms of their human capital development stimulated by RPS and measured by nutritional status and schooling enrolment. The second subsection goes on to see what role RPS has had during this downturn on an even wider set of indicators. The next subsection takes up the question of transitions into and out of coffee and the final subsection describes a series of informal robustness tests carried out.

The Changing Environment: Control Group

I first consider patterns of expenditures and labor force participation in the control areas during the period 2000–2002, contrasting coffee and noncoffee growing areas. In the top panel of Table 6, I present descriptive results for expenditure and labor force participation measures over time for the control group. Real per capita annual household total expenditures measured in base year 2000 córdobas dropped by nearly 20 percent between 2000 and 2001, but held steady between 2001 and 2002.[11,12] A similar pattern, but with a larger percentage decline, is observed for per capita household food expenditures. At the same time that expenditures were declining, labor supply was increasing, as measured by the total number of hours worked by household members and the average number of hours worked per worker (shown in the third and fourth columns, respectively). In 2001 and 2002, workers reported working, on average, more than 1

[11] All reported expenditures have been deflated to 2000 base córdobas using the Nicaraguan consumer price index reported by the Central Bank of Nicaragua for which there was approximately 4 percent inflation per year in 2001 and 2002.

[12] The drop in expenditures in the control group was not due to changes in household size or family composition, which did not significantly change. Another possibility is that there are biases in the reporting of expenditures. For example, in control areas, it is possible that nonbeneficiaries who had learned about the program understated their expenditures in an effort to appear more in need of the program. However, at this stage, the program was being implemented using only geographical targeting, and being more or less poor would not have affected eligibility. Additional evidence that the decline in expenditures is real comes from the decline in nutritional status of children, which is not subject to the same sort of possible reporting bias.

additional hour a week, compared to in 2000. Households in the control group faced declining expenditures despite increased labor hours. (Appendix Table 12 presents selected sample means.)

Table 6—Expenditures and labor force participation in the control group, 2000–2002

	Ln per capita real annual expenditure	Ln per capita real annual food expenditure	Total hours worked last week	Average hours per worker worked last week
Year 2001	-0.1895 ***	-0.2473 ***	2.0391	1.4082 ***
	(7.50)	(8.55)	(1.03)	(2.92)
Year 2002	-0.1767 ***	-0.2331 **	5.9565 ***	1.1056 **
	(6.99)	(8.06)	(3.01)	(2.29)
Constant	8.0166 ***	7.6370 ***	84.1884 ***	25.5777 ***
	(448.9)	(373.6)	(60.25)	(74.97)
Year 2001	-0.0928 ***	-0.1324 ***	-0.1701	-0.0088
	(2.77)	(3.46)	(0.07)	(0.01)
Year 2002	-0.1053 ***	-0.1661 ***	-3.6031	-1.7101 ***
	(3.15)	(4.34)	(1.38)	(2.70)
Year 2001 × coffee	-0.2208 ***	-0.2626 ***	5.0476	3.2377 ***
	(4.37)	(4.54)	(1.28)	(3.38)
Year 2002 × coffee	-0.1629 ***	-0.1531 ***	21.8415 ***	6.4331 ***
	(3.22)	(2.64)	(5.55)	(6.72)
Constant	8.0166 ***	7.6370 ***	84.1884 ***	25.5777 ***
	(451.9)	(376.1)	(60.95)	(76.14)
F-test Year 2001 + Year 2001 × coffee	68.41 ***	82.80 ***	2.73 *	20.22 ***
	[<0.01]	[<0.01]	[0.10]	[0.07]
Joint test year 2002 + Year 2002 × coffee	50.05 ***	54.06 ***	38.14 ***	43.25 ***
	[< 0.01]	[<0.01]	[<0.01]	[<0.01]
F-test overall regression	22.94 ***	28.57 ***	10.83 ***	13.72 ***
	[<0.01]	[<0.01]	[<0.01]	[<0.01]
Number of observations	2,070	2,070	2,070	2,070

Notes: Household-level fixed-effects estimation in control group only. T-statistics reported in parentheses and p-values in brackets. * indicates significance at 10 percent, ** indicates significance at 5 percent, and *** indicates significance at 1 percent.

An important consideration is whether these patterns hold for both poor and nonpoor households. To examine this, I consider two separate categorizations of households, one using their predicted poverty status based on a proxy means model that predicts per capita expenditures for each household based on a set of indicators measured at the household level that are highly correlated with logarithmic per capita expenditures ($R^2 > 0.50$), and a second based on the size of their landholdings being less or greater than one hectare.[13] Both of these show that while expenditures decreased for both poor and nonpoor groups over the period, the decline was concentrated in nonfood expenditures and sharper for the less poor, the group that may have been better able to withstand reductions; the labor force participation trends were similar across groups (results not shown).

There were two possible factors leading to a downturn in the area, a drought in 2001 and the low coffee prices. In the 2001 *comarca*-level survey, 38 of 42 *comarcas* reported the drought as a significant event during the past year, indicating that it was pervasive in the program areas. The fact that expenditures did not recover in 2002, however, suggest that the downturn observed was not due solely to drought. To explore whether the decline was specifically related to coffee, I compare coffee- and noncoffee-growing areas (the bottom panel of Table 6). Households in coffee-growing areas started out with somewhat higher (by about 10 percent) expenditures, but this advantage was reversed over the period as they experienced significantly larger declines in expenditures. Average per capita expenditures in the noncoffee-growing areas were about 10 percent lower in 2001 and 2002 than in 2000, but a further 20 percent lower in coffee-growing regions. Total hours worked in the last week by household members increased modestly in 2001, by about 5 hours per week, and substantially in 2002, about 18 hours per week. Average hours per worker in the last week increased in coffee-growing areas in both 2001 and 2002. Changes in households in coffee-growing areas are driving the overall average trends depicted in the first panel of Table 6.

I next consider how school enrolment rates and child labor have changed over time in the control group. Since schooling and child labor decisions depend on the opportunity cost of children's time as well as costs of schooling and the resources the household commands, it is not possible a priori to predict the direction of the effects of an economic downturn, since opportunity costs and resources may both be changing, with

[13] Predicted poverty status is used rather than actual measured expenditure poverty since the latter is likely to lead to regression to the mean given measurement errors in expenditures. IFPRI (2002) contains the details.

opposing influences for household decisions. In 2000, though less than 20 percent reported working, children ages 7–12 were more likely to report having worked in the last week in coffee-growing areas versus noncoffee-growing areas (19 versus 12 percent). Possibly reflecting these differing work patterns, net primary enrolment rates for the same children were substantially lower in coffee-growing areas (66 percent versus 87 percent). (See Appendix Table 12.)

In the first two columns of Table 7, I present household-level fixed-effects estimates of the changes in enrolment rates for girls and boys over time in the control group, conditional on age in years. Enrolment rates were substantially higher in 2000 for girls (83 percent) than for boys (74 percent). For both girls and boys, there was hardly any change from 2000 to 2001, but enrolment rates were up significantly for both groups in 2002 (relative to 2000), and more so for boys, who made relative gains over the period. Turning to the bottom panel of the table, which again distinguishes between coffee- and noncoffee-growing areas, one sees that most of the gains over the period were concentrated in coffee growing areas; by 2002, about one-third of the gap between net primary enrolment rates that existed in 2000 between coffee- and noncoffee-growing areas had been overcome.

While girls in this age group rarely reported working (on average, less than 10 percent do), about one-quarter of the boys ages 7–12 reported working in 2000. By 2002, however, this had declined to about 15 percent in both coffee- and noncoffee-growing areas (see the third and fourth columns of Table 7). The same pattern holds for older children in the sample between ages 13–17 (not shown). It would seem that the downturn did not adversely affect enrolment and, if anything, had negative effects on the incidence of child labor for young children, possibly because of reduced labor demand.

One concern with the above analysis relates to the timing of the surveys, since the baseline was carried out in August/September and the follow-up surveys in October. It is possible that seasonal variation in consumption or work could lead to part or all of the observed changes. Indeed, when broken down by recall period, the higher frequency periods show declines in expenditures across the surveys, but the longer recall periods (that include nonfood items) of 1 month, 6 months, and 12 months, do not. If all

Table 7—Primary enrolment, child labor, and child nutritional status in the control group, 2000–2002

	(1) if 7–12 year old enrolled:		(1) if 7–12 year old working:		HAZ children 6–48 months of age
	Girls	Boys	Girls	Boys	
Year 2001	-0.0107	0.0004	-0.0094	-0.0953 ***	
	(0.51)	(0.02)	(0.65)	(4.11)	
Year 2002	0.0468 **	0.0701 ***	-0.0178	-0.0933 ***	-0.1480 *
	(2.09)	(2.87)	(1.15)	(3.74)	(1.77)
Age in years (months in final column)	0.0133 **	0.0202 ***	0.0191 ***	0.0741 ***	-0.0045
	(1.98)	(2.67)	(4.12)	(9.63)	(1.32)
(1) if male	n/a	n/a	n/a	n/a	0.0613
					(0.73)
Constant	0.6997 ***	0.5581 ***	-12.8903 ***	-0.4558 ***	-1.6337 ***
	(11.40)	(8.06)	(3.03)	(6.46)	(13.93)
Year 2001	-0.0139	-0.0288	-0.0057	-0.0772 ***	
	(0.51)	(0.97)	(0.31)	(2.54)	
Year 2002	0.0407	0.0067	-0.0401 **	-0.0870 ***	-0.0639
	(1.42)	(0.21)	(2.03)	(2.68)	(0.63)
Year 2001 × coffee	0.0079	0.0712	-0.0077	-0.0428	
	(0.19)	(1.57)	(0.27)	(0.92)	
Year 2002 × coffee	0.0149	0.1457 ***	0.0551 *	-0.0163	-0.1616
	(0.34)	(3.12)	(1.83)	(0.34)	(1.37)
Age in years (months in last column)	0.0132 **	0.0205 ***	0.0186 ***	0.0743 ***	-0.0046
	(1.97)	(2.72)	(4.02)	(9.64)	(1.35)
(1) if male	n/a	n/a	n/a	n/a	0.0684
					(0.82)
Constant	0.7002	0.5538 ***	-0.1247 ***	-0.4563 ***	-1.6347 ***
	(11.39)	(8.03)	(2.94)	(6.46)	(13.95)
F-test Year 2001 + Year 2001 × coffee	0.04	1.49	0.37	11.41 ***	
	[0.85]	[0.22]	[0.54]	[<0.01]	
Joint test Year 2002 + Year 2002 × coffee	2.66 *	18.01 ***	0.41	7.88 ***	2.38 *
	[0.10]	[<0.01]	[0.52]	[<0.01]	[0.09]
F-test overall regression	3.21 ***	7.19 ***	4.53 ***	20.28 ***	1.84
	[<0.01]	[<0.01]	[<0.01]	[<0.01]	[0.12]
Number of observations	1,196	1,190	1,196	1,190	774

Notes: Household-level fixed-effects estimation in control group only for first four columns; ordinary least squares estimation with robust standard errors allowing for heteroscedasticity in the final column (Stata Corporation 2001). T-statistics are reported in parentheses and p-values in brackets. * indicates significance at 10 percent, ** indicates significance at 5 percent, and *** indicates significance at 1 percent.

periodicities, including the longer recall periods, showed a decline, one could say more confidently that the observed declines are not due to seasonal variation in expenditures.[14]

The first piece of evidence I bring to bear on whether the results presented in Table 6 are due solely to seasonality comes from an independent source of information, a quality control survey carried out on a 5 percent sample of the households interviewed in the 2000 baseline. This survey was implemented in October 2000—so that the timing exactly matches the follow-up surveys. The estimate of the mean and median change in the logarithm of per capita expenditures and per capita food expenditures shows that they increased slightly over the period—in both coffee- and noncoffee-producing areas, though these increases are not statistically significant. A comparison of number of workers, total hours worked, and average hours worked per worker also show slight (but insignificant) increases. Thus, at least in 2000, the baseline survey year, there was no dramatic decline in expenditures between August and October, supporting the interpretation that the changes seen between 2000 and 2001/2002 are real changes resulting from the economic downturn.

The second piece of evidence supporting the hypothesis that the downward trend in expenditures reflects a real economic downturn and not merely seasonality is shown in the final column of Table 7, where I present the findings for height-for-age Z-scores of children ages 6–48 months of age.[15] Due to planning difficulties, the anthropometry survey in 2000 was carried out separately from the main household survey work—in September and early October 2000. Thus, for anthropometry, the 2000 and 2002 surveys were implemented closer together, so that seasonal variation is not a concern when comparing them. There was a significant decline in the nutritional status of children in the control areas over the period, and this decline appears to have been more severe for households in coffee-growing regions (see joint F-test in third to bottom row). When

[14] Of course, for seasonal variation to be driving the difference between coffee and noncoffee areas, there would also need to be different patterns of seasonal variation between the groups.

[15] Unlike the other regressions reported in Table 7, the height-for-age Z-score specification is not estimated using household-level fixed effects, because the sample for which there is a child between the age of 6 and 48 months from the same household measured in both 2000 and 2002 is too small for precise estimation.

broken down by sex, the height-for-age Z-scores for boys, which, on average, was slightly higher than girls at the outset, deteriorated more severely with the result that in 2002, the two were nearly identical (controlling for age)—none of these differences by sex are statistically significant, however.

To summarize, the evidence shows that expenditures have declined over the period while labor supply has increased, and these changes are, on average, substantially larger in coffee-growing areas. Nevertheless, primary enrolment rates improved modestly over the period, somewhat more so within coffee-growing areas, and the incidence of child labor for young boys declined in all areas.

Effect of the RPS on Households in Coffee *Comarcas*

Governmental responses in Central America to the decline in coffee prices, including those of the Nicaraguan government, were slow to materialize and initially have focused attention on producers, traders, and exporters, rather than laborers, even though it is the latter who appear to be more vulnerable. Further, because the initial responses tended to be directed via the financial sector, they favored medium and large enterprises to the detriment of the small-scale producers prevalent in Nicaragua.[16] Since many of these households also cultivate other crops, and the downturn in prices was accompanied by a drought (at least in 2001), their livelihoods were doubly threatened (Varangis et al. 2003).

Varangis et al. (2003) outline a variety of possible responses to the decline in prices, ranging from price risk management instruments (see McCarthy and Sun 2003 for a discussion of these in Honduras) to food-for-work programs. They call for improving social safety net programs, of which RPS is one example, making this analysis complementary to theirs. One recently begun Nicaraguan program they describe is *Plan Café*, which aims to help both large producers and laborers alike. Participants are employed on private coffee farms and are paid, in part, by the farm owners and in part by

[16] Varangis et al. (2003) estimate that 90 percent of producers in Nicaragua produce less than 100 quintals.

the government, in the form of food. It turns out, however, that this program was never very widely implemented. The results presented below should be interpreted as what happens in the absence of a major governmental response.

Section 2 described the average effects of RPS on a variety of outcomes. In this section, I demonstrate that RPS has had greater average impacts in coffee-growing versus noncoffee-producing regions, for many indicators. Of course, this is not surprising, since there was more “potential” for the program to have impact where the situation was worse or deteriorating more rapidly. This is similar in spirit to the general finding in the overall RPS evaluation that double difference estimated average impacts tend to be larger among the poorer groups in the sample, where there was often more potential, for example, due to lower enrolment rates among the extreme poor (Maluccio and Flores 2004).

In both years, the program positively and significantly improved per capita total annual household expenditures and per capita household food expenditures. Across all program areas, RPS increased these expenditure measures by nearly 20 percent, on average (see Table 8). In 2001, the program effect in coffee-producing areas was substantially larger than in noncoffee areas. This differential, however, was substantially smaller in 2002 where, again, the program had a significant impact on expenditures, though it was not significantly larger in coffee- versus noncoffee-growing areas. As I argue below, this may reflect the increased labor supply between 2001 and 2002 by nonprogram recipients in coffee-growing areas.

Overall, the program had little significant effect on either total number of hours worked last week or hours worked per worker, but within coffee-growing areas, it had a negative effect on both.[17] These effects were larger in 2002 than in 2001, possibly explaining the weaker program effect on expenditures in that year as households without the program worked harder to make up for lost consumption. The estimated effects are

[17] The program also did not affect the number of adult household members nor did it have effects on individual migration. Notice that unlike the discussion earlier regarding a concern that the timing of the survey may affect changes across rounds, the double difference estimator controls for this possibility, so it is not a concern here.

driven largely by male labor, which comprises about 90 percent of the total reported labor; excluding women does not change the findings and estimating the relationship for women alone leads to similar conclusions. The negative estimated impact on labor supply does not, however, reflect a large decline in labor supply for program beneficiaries, which dropped about 8 hours per week in 2001 but only 2 hours a week in 2002, but rather reflects the substantial increase in hours worked by their coffee region counterparts who are not beneficiaries. In the absence of the program, then, beneficiary households in coffee-producing regions would have had to devote substantially more

Table 8—The effect of Nicaraguan RPS on expenditures and labor force participation, 2000–2002

	Ln per capita real annual expenditures	Ln per capita real annual food expenditures	Total hours worked last week	Average hours per worker worked last week
Year 2001	-0.0928 ***	-0.1324 ***	-0.1701	-0.0088
	(2.92)	(3.57)	(0.07)	(0.01)
Year 2002	-0.1053 ***	-0.1661 ***	-3.6031	-1.7101 ***
	(3.31)	(4.48)	(1.45)	(2.85)
Year 2001 × coffee	-0.2208 ***	-0.2626 ***	5.0476	3.2377 ***
	(4.59)	(4.69)	(1.35)	(3.57)
Year 2002 × coffee	-0.1629 ***	-0.1531 ***	21.8415 ***	6.4331 ***
	(3.39)	(2.73)	(5.82)	(7.09)
Year 2001 × RPS area	0.1816 ***	0.2781 ***	-3.9191	-0.4825
	(4.02)	(5.28)	(1.11)	(0.57)
Year 2002 × RPS area	0.1749 ***	0.2618 ***	0.3406	0.7732
	(3.97)	(4.97)	(0.10)	(0.91)
Year 2001 × coffee × RPS area	0.2789 ***	0.2553 ***	-13.0845 **	–4.2388 ***
	(4.14)	(3.25)	(2.49)	(3.33)
Year 2002 × coffee × RPS area	0.0657	0.0561	-23.4683 ***	–5.2571 ***
	(0.97)	(0.71)	(4.46)	(4.13)
Constant	8.0599 ***	7.6714 ***	81.9047 ***	25.2518 ***
	(679.85)	(555.47)	(88.56)	(112.87)
F-test Year 2001 + Year 2001 × coffee	84.58 ***	83.61 ***	18.95 ***	24.97 ***
	[<0.01]	[<0.01]	[<0.01]	[<0.01]
Joint test Year 2002 + Year 2002 × coffee	23.99 ***	29.71 ***	35.06 ***	22.52 ***
	[<0.01]	[<0.01]	[<0.01]	[<0.01]
F-test overall regression	17.31 ***	19.19 ***	8.88 ***	8.97 ***
	[<0.01]	[<0.01]	[<0.01]	[<0.01]
Number of observations	4,188	4,188	4,188	4,188

Notes: Household-level fixed-effects estimation in control group only. T-statistics reported in parentheses and p-values in brackets. * indicates significance at 10 percent, ** indicates significance at 5 percent, and *** indicates significance at 1 percent.

time to work (and at the same time would have suffered declines in per capita expenditures).

In a separate section of the questionnaire, households report on remittances received over the past year. In regressions similar to those in Table 8 (but not shown), where the dependent variable is an indicator of whether a household received remittances in the past year (or the amount of remittances received), I find that RPS had a negative effect on the probability a household received remittances in 2001—but only in noncoffee growing areas. In coffee-growing areas, which underwent a more severe decline over the period, the program had no significant effect on receipt of remittances.

Despite coming with conditionality that may substantially increase private costs to households,[18] RPS transfers likely relax beneficiaries' budget constraints, allowing them to re-optimize and thereby improve both their current and future situations. Given the long-term downward trend in coffee prices, one might expect to see exit from coffee (if, indeed, it has not already begun before this survey began) over the medium-to-long term, fixed costs for coffee production notwithstanding. If, for example, households are credit constrained, they may not be able to reallocate their activities immediately, and so remain in coffee. It is possible that access to the additional resources provided by RPS allow this credit constraint to be broken and, in addition to changes in hours worked, one would see changes in the type of work being carried out.

A separate credit constraint pathway via which RPS may work is posited in Coady, Olinto, and Cáldes (2003). Presenting a simple two-period model for small coffee farmers, they demonstrate how unconditional transfers can have a direct income effect on labor supply (for all households) but also an indirect effect for credit-constrained coffee farmers who, instead of having to seek off-farm labor, are able to devote more time to maintaining their coffee trees, thereby raising the marginal productivity of their coffee land. That the transfers are conditioned on child attendance at school introduces a third effect, substitution between child and adult labor. Finally,

[18] Caldés and Maluccio (2005) provide some estimates of private costs for beneficiary women of around 40 hours per year and C$40 in additional transportation costs.

although not mentioned in their work, a related aspect to conditionality is that it might keep some children and adults from (temporarily) migrating, thus increasing (relative to without the program) the availability of local labor. If these are the underlying mechanisms, in contrast to the argument in the previous paragraph, one would see more labor devoted to coffee, rather than less.

In the baseline 2000, fully three-quarters of the households (in both coffee- and noncoffee-growing areas alike) indicated that they were credit constrained in the sense that either they had requested a loan (from either formal or informal sources) but not received it, or that they had not requested a loan, but did not do so because they felt they would not receive it. Because of the predominance of credit-constrained households, I do not report results distinguishing program effects between whether a household was credit constrained or not before the program, noting that any time-invariant components of differences between these types of households is already controlled for in the analysis. When the extent to which results differ for credit-constrained versus credit-unconstrained households is considered, I find that most effects tend to be slightly larger for credit-constrained households, but not significantly so.

I now examine the types of work households carry out with and without the program to see if in addition to changes in total hours, the program induces adjustments along other dimensions of labor supply. The results are presented in Table 9. In the first column, I assess the impact on total hours dedicated to agriculture in the last week—RPS reduced the total number of hours dedicated to agriculture, on average, for coffee-producing areas, by around 10 hours a week. Nonetheless, despite these large declines, when I consider the fraction of labor hours in the household dedicated to agriculture, the RPS effect was negative for households in noncoffee-growing areas, but positive for households in coffee-growing areas in 2002. (Clearly, the effect of RPS on total hours was also negative and larger than that on agricultural hours alone, in coffee-growing areas.) The evidence for small business participation is consistent with these patterns—program beneficiaries in coffee-growing *comarcas* are less likely to be undertaking small business activities than their counterparts in nonbeneficiary *comarcas*.

Table 9—The effect of Nicaraguan RPS on occupational choice, 2000–2002

	Total hours dedicated to agriculture last week	Fraction of labor allocated to agriculture last week	(1) if small business activity last week	(1) if regular small business activity
Year 2001	1.3119	0.04389 ***	-0.2113 ***	0.0515 ***
	(0.58)	(2.60)	(7.62)	(2.60)
Year 2002	4.3686 **	0.1026 ***	-0.2010 ***	-0.0309
	(1.95)	(6.07)	(7.25)	(1.56)
Year 2001 × coffee	2.1848	-0.0245	0.1352 ***	-0.0085
	(0.64)	(0.96)	(3.22)	(0.28)
Year 2002 × coffee	7.8036 **	-0.1194 ***	0.1348 ***	0.0674 **
	(2.30)	(4.69)	(3.21)	(2.25)
Year 2001 × RPS area	-2.1229	-0.0053	0.1011 ***	-0.0568 **
	(0.67)	(0.22)	(2.57)	(2.02)
Year 2002 × RPS area	-4.0562	-0.0633 ***	0.0619	0.0021
	(1.27)	(2.63)	(1.57)	(0.07)
Year 2001 × coffee × RPS area	-11.5277 **	-0.0297	-0.1665 ***	–0.0663
	(2.42)	(0.82)	(2.83)	(1.58)
Year 2002 × coffee × RPS area	-9.6113 **	0.1208 ***	-0.0972 *	–0.1339 ***
	(2.02)	(3.36)	(1.65)	(3.18)
Constant	65.1655 ***	0.8094 ***	0.1841 ***	0.1218 ***
	(77.90)	(128.28)	(17.80)	(16.47)
F-test Year 2001 + Year 2001 × coffee	14.93 ***	1.70	2.24	15.53 ***
	[<0.01]	[0.19]	[0.13]	[<0.01]
Joint test Year 2002 + Year 2002 × coffee	14.97 ***	4.66 **	0.65	17.82 ***
	[<0.01]	[0.03]	[0.42]	[<0.01]
F-test overall regression	6.07 ***	7.16 ***	16.42	5.83 ***
	[<0.01]	[<0.01]	[<0.01]	[<0.01]
Number of observations	4,188	4,188	4,188	4,188

Notes: Household-level fixed-effects estimation in control group only. T-statistics reported in parentheses and p-values in brackets. * indicates significance at 10 percent, ** indicates significance at 5 percent, and *** indicates significance at 1 percent.

If staying in agriculture were equivalent to staying in coffee, this evidence would suggest that households in coffee-growing beneficiary *comarcas* may actually be intensifying their involvement in coffee. It may reflect how in breaking the credit constraint, coffee-producing households are able to invest more labor in their coffee-related activities (before the harvest) to improve returns, as posited by Coady, Olinto, and Caldés (2003). Evidence on transitions into and out of coffee is considered in the following subsection.

When I consider program effects dividing the sample as above by predicted poverty and, separately, landownership, on the whole, the findings above are unchanged though, as in the impact evaluation, nearly all of the effects were larger in magnitude for poorer households (though rarely significantly so). An exception occurs when I consider total and food expenditures and categorize by landownership. For these outcomes, estimated effects in coffee-growing areas are larger for those with more than 1 hectare of land; this is consistent with the possibility that some of these households cultivate coffee and were more severely affected by the downturn.

The first subsection of this section showed that changes in primary enrolment and child labor also varied according to whether the child lives in a coffee- or noncoffee-growing area. Unsurprisingly, then, I also find differences in RPS effects across the two types, as shown in Table 10. Program effects on enrolment rates of girls age 7–12 were negligible in noncoffee-growing areas but quite substantial, more than 20 percentage points, in coffee-growing areas. This reflects the large gap between coffee- and noncoffee-growing areas that existed before the program (and still exists in the control group). With the combination of the transfers and the conditionality, RPS has essentially equalized enrolment rates for this group across the areas. For boys, the effect was more evenly spread among coffee- and noncoffee-growing areas and only in 2001 were they significantly larger in the latter areas. When I consider current attendance in school (defined as having missed fewer than six days in the previous two months of school) in results not shown, the pattern for girls is the same, but for boys, there are positive and significant effects in all areas with substantially larger ones in coffee-growing areas.

Consistent with the large increase in enrolment (though not necessary, since simultaneously working and attending school is common), RPS had a substantial negative effect on girls ages 7–12 working in the last week, but only in coffee growing areas. There were no significant program effects on boys in this age group, though all the estimated coefficients were negative. Overall, RPS did have significant negative effects on the incidence of child labor for boys; these insignificant effects are likely the result of splitting the sample into coffee and noncoffee households and also reflect the fact that schooling and work are not mutually exclusive. When I consider separately poor and

Table 10—The effect of Nicaraguan RPS on primary enrolment, child labor, and child nutritional status in the control group, 2000–2002

	(1) if 7–12 year old enrolled		(1) if 7–12 year old working		HAZ children 6-48 months of age
	GIRLS	BOYS	GIRLS	BOYS	
Year 2001	-0.0141	-0.0269	-0.0054	-0.0701 **	
	(0.52)	(0.97)	(0.30)	(2.51)	
Year 2002	0.0403	0.0117	-0.0395 **	-0.0691 **	-0.0383
	(1.43)	(0.40)	(2.08)	(2.35)	(0.42)
Year 2001 × coffee	0.0835 **	0.1083 ***	-0.0025	0.0452	
	(2.10)	(2.72)	(0.09)	(1.13)	
Year 2002 × coffee	0.0332	0.0581	0.0007	-0.0118	-0.1625
	(0.80)	(1.40)	(0.02)	(0.28)	(1.38)
Year 2001 × RPS area	0.0079	0.0716 *	-0.0077	-0.0415	
	(0.19)	(1.68)	(0.27)	(0.97)	
Year 2002 × RPS area	0.0148	0.1454 ***	0.0553 *	-0.0175	0.3575 ***
	(0.34)	(3.32)	(1.89)	(0.40)	(3.05)
Year 2001 × coffee × RPS area	0.2463 ***	0.1011 *	-0.0837 **	-0.0836	
	(4.18)	(1.72)	(2.10)	(1.41)	
Year 2002 × coffee × RPS area	0.2336 ***	0.0557	-0.1061 ***	-0.0469	-0.2408
	(3.82)	(0.91)	(2.57)	(0.76)	(1.43)
Age in years (months in final column)	0.0136 ***	0.0157 ***	0.0178 ***	0.0573 ***	-0.0087 ***
	(2.76)	(3.21)	(5.35)	(11.62)	(3.62)
(1) if male	n/a	n/a	n/a	n/a	0.0450
					(0.77)
Constant	0.6603 ***	0.6155 ***	-0.1036 ***	-0.3415 ***	-1.5285 ***
	(14.72)	(13.68)	(3.42)	(7.54)	(18.51)
F-test Year 2001 + Year 2001 × coffee	57.43 ***	23.22 ***	8.57 ***	0.77	
	[<0.01]	[<0.01]	[<0.01]	[0.38]	
Joint test year 2002 + Year 2002 × coffee	35.12 ***	6.38 ***	12.01 ***	1.68	0.95
	[<0.01]	[0.01]	[<0.01]	[0.20]	[0.33]
F-test overall regression	20.89 ***	18.56 ***	6.35 ***	18.12 ***	5.58 ***
	[<0.01]	[<0.01]	[<0.01]	[<0.01]	[<0.01]
Number of observations	2,359	2,430	2,359	2,430	1,493

Notes: Household-level fixed-effects estimation in control group only for first four columns; ordinary least squares estimation with robust standard errors allowing for heteroskedasticity in the final column (Stata Corporation 2001). T-statistics are reported in parentheses and p-values in brackets. * indicates significance at 10 percent, ** indicates significance at 5 percent, and *** indicates significance at 1 percent.

nonpoor households, it turns out that as discussed earlier, the larger effects are concentrated among the poorer households (regardless of classification method).

Finally, the effect on child height-for-age Z-scores seems to be less positive in coffee-growing areas (net positive effect of 0.12 compared with 0.36 in noncoffee growing areas) and is not significant, though this may also be in part due to the smaller sample sizes being considered.

Transitions into and out of the Coffee Sector

I next consider whether and how RPS influenced the extent to which households were moving in and out of the coffee "industry," using the available, albeit imperfect, individual-level information regarding what persons did as their main activity in the last week described in Section 3 above. Table 11 presents results from regressions in which the dependent variable is an indicator of whether anyone in the household indicated they worked in the coffee sector in the last week (first column), whether the participation was as a laborer (second column), or whether the participation was as a producer. In the first column, while it is clear that participation was lower in 2001 and 2002 relative to 2000 (in coffee-growing areas), there does not seem to have been any effect of RPS on household participation in the coffee sector. When I split participation into those participating as laborers and those participating as producers, however, it appears that program effects are significant—but have opposite effects on laborers and producers. The program decreases participation as coffee laborers but has a positive effect on participation as producers. Since, on average, there were fewer producers in 2002 than in 2000, what these effects mean is that beneficiary households were more likely to remain as coffee producers than their counterparts without the program (though recall that participation is only a small percentage and that these data must be treated carefully).

Program beneficiaries in coffee-growing areas appear to be less likely to have been working in coffee as laborers but more likely as producers. Taken together with the results on the share of work in agriculture, it suggests that those households who are not coffee producers are intensifying activity in other agricultural activities, including maize and bean cropping, though it is not possible at this point to say which ones. The findings are also consistent with the existence of credit constraints inhibiting such transitions, to the extent that agricultural activities require start-up investments and do not yield returns

Table 11—The effect of Nicaraguan RPS on participation in the coffee sector at the household level, 2000–2002

	(1) if any participation in coffee sector	(1) if laborer participation in coffee sector	(1) if producer participation in coffee sector
Year 2001	-0.0103	-0.0103	-0.0026
	(0.56)	(0.58)	(0.29)
Year 2002	-0.0129	-0.0129	-0.0026
	(0.71)	(0.73)	(0.29)
Year 2001 × coffee	-0.0592 **	-0.0327	-0.0438 ***
	(2.15)	(1.22)	(3.25)
Year 2002 × coffee	-0.0732 ***	-0.0700 ***	-0.0305 **
	(2.65)	(2.61)	(2.27)
Year 2001 × RPS area	0.0051	0.0051	0.0026
	(0.19)	(0.20)	(0.20)
Year 2002 × RPS area	-0.0081	-0.0055	-0.0001
	(0.31)	(0.22)	(0.00)
Year 2001 × coffee × RPS area	-0.0463	-0.0666 *	0.0315 *
	(1.20)	(1.77)	(1.66)
Year 2002 × coffee × RPS area	-0.0043	-0.0225	0.0239
	(0.11)	(0.60)	(1.27)
Constant	0.1447 ***	0.1297 ***	0.0301
	(21.25)	(19.62)	(9.05)
F-test Year 2001 + Year 2001 × coffee	2.06	4.87 **	5.88 **
	[0.15]	[0.03]	[0.02]
Joint test year 2002 + Year 2002 × coffee	0.18	1.01	2.89 *
	[0.67]	[0.32]	[0.09]
F-test overall regression	7.31 ***	7.50 ***	3.03 ***
	[<0.01]	[<0.01]	[<0.01]
Number of observations	4,188	4,188	4,188

Notes: Household-level fixed-effects estimation in control group only. T-statistics reported in parentheses and p-values in brackets. * indicates significance at 10 percent, ** indicates significance at 5 percent, and *** indicates significance at 1 percent.

for some time—in contrast to some of the nonagricultural activities are more likely yielding immediate returns. Furthermore, if the beneficiary households are optimizing, then it suggests returns to noncoffee agricultural activities are higher than both coffee and the available nonagricultural activities in coffee-growing regions. For those households that are coffee producers, it would appear that RPS is serving as a risk-coping mechanism during the crisis, allowing them not to exit (in comparison to their counterparts in coffee-growing areas without the program), consistent with the results from Coady, Olinto, and Caldés (2003).

Robustness of the Results

As described at the beginning of this section, the above results are estimated using the balanced panel household sample of 1,396 households and controlling for household fixed effects. In this section, I consider whether the results change when I account for some statistical concerns for these data.

If one is willing to ignore the household fixed effects, I can more conservatively estimate the standard errors to better control for heteroskedasticity and *comarca*-level clustering. Doing this by estimating robust standard errors allowing for clustering (Stata Corporation 2001) changes the estimated effects very little on the whole, but does increase the estimated standard errors, thereby reducing significance. Nonetheless, none of the substantive findings change. Above I also ignored the stratified sample design, which can be corrected for statistically using sample weights; correcting for this aspect of the design also requires that I ignore the household fixed effects. As above, the results change very little.

While the household-level fixed-effects approach controls for attrition biases associated with fixed household characteristics, attrition is still a concern, since it is possibly related to time-varying factors that may also be directly related to the outcomes being considered. While I do not implement an attrition selection correction procedure, as an additional check that attrition is not leading to severely biased results, I re-estimate the above analyses using the unbalanced sample, and find no substantive differences in the findings. Recall that the number of households is about evenly divided between intervention and control groups, suggesting that attrition was not significantly different between intervention and control groups. I therefore conclude that attrition bias is not driving the results presented here.

When I re-estimate all the results excluding *comarcas* from the department of Madriz (where coffee is not grown), the results are similar in magnitude throughout—though at times no longer significant because of the loss of power in reducing the sample size by about one-third. Finally, when controls are included for the severity of the drought (which did vary across the sample even though nearly all *comarcas* reported having been effected by the drought), little changes—this is likely due to the fact that the fixed effects already control for much of the potential bias associated with drought. The effects of the drought are not being conflated with those due to the downturn in coffee prices.

5. Conclusions

A major cause of the intergenerational transmission of poverty is the inability of poor households to invest in the human capital of their children. Supply-side interventions, which increase the availability and quality of health and education services, are often ineffective in resolving this problem, since the resource constraints facing poor households preclude them from shouldering the private costs associated with utilizing these services (e.g., travel costs and the opportunity cost of women's and children's time). Innovative programs like RPS attack this problem by targeting transfers to the poorest communities and households, and by conditioning these transfers on attendance at school and health clinics. This effectively transforms pure transfers into human capital subsidies for poor households. An evaluation of the Nicaraguan RPS has shown that this approach was largely successful against its primary objectives of supplementing income to increase expenditures on food, improving school matriculation, and improving child health care.

The international and local Nicaraguan media have widely reported about the "coffee crisis" in Central America. As the crisis should probably not be considered a surprise and the sharp downturn in prices occurred before the data were collected, to a certain extent the estimates presented in this paper represent lower bounds. Nevertheless, the evidence shows that there has been a downturn in Nicaragua over the period and that it has been more severe in the coffee-growing regions of RPS. In noncoffee-growing areas, per capita expenditures declined by 10 percent over the 2000–2002 period and by more than twice that in coffee-growing areas. Nutritional status of children ages 6–48 months also deteriorated over the period. Educational outcomes and child labor, however, did not worsen, possibly reflecting decreased rural labor demand. Households responded to the crisis in part by increasing labor supply.

As a result, RPS had generally larger effects in coffee-growing areas than in noncoffee-growing areas. While not designed as a safety net program in the sense of reacting to crises or shocks, RPS has performed more like such a traditional safety net program, protecting more those who were most affected by the downturn. For example, it provided a cushion for per capita expenditures, enabling beneficiary households to maintain pre-program expenditure levels in both coffee- and noncoffee-growing areas. Given the differences in the severity of the crisis across the areas, this means that the program had substantially larger estimated effects in coffee-growing areas. It also protected, and promoted, investment in child human capital (as indicated by increased enrolment rates, decreased child labor, and improved height-for-age Z-scores). The co-responsibilities were not abandoned, showing that conditional cash transfer programs can be effective (and even more effective) during a downturn. Overall, the program had little significant effect on either total number of hours worked last week or hours worked per worker, but within coffee-growing areas, it had a negative effect on both. While not depressing labor supply relative to before the program, RPS also seems to have muted additional labor supply for beneficiaries in coffee-growing areas (relative to their counterparts without the program).

The evidence is more mixed, however, as to whether and how RPS enabled households to reallocate their resources in response to trends in coffee prices—in part because of data limitations. RPS reduced the total number of hours dedicated to agriculture, on average, for coffee-producing areas, but despite this decline, these same households increased the proportion of their total labor supply dedicated to agriculture. The findings for small business participation are consistent with these patterns—program beneficiaries in coffee-growing *comarcas* are less likely to be undertaking small business activities than their counterparts in nonbeneficiary *comarcas*.

If staying in agriculture were equivalent to staying in coffee, this evidence would suggest that households in coffee-growing beneficiary *comarcas* may be intensifying their involvement in coffee. To more directly explore these hypotheses, transitions into and out of coffee were examined: beneficiaries who participated in coffee (before the program) as laborers were more likely to be exiting (or at least working less in) the coffee industry, whereas those who participated as producers were less likely to be exiting.

Taken together with the findings on the share of work in agriculture, these results suggest that those households who are not coffee producers are intensifying activity in other agricultural activities, though it is not possible to say which ones. The findings are also consistent with the existence of credit constraints inhibiting such transitions in the absence of the program, to the extent that agricultural activities require start-up investments and do not yield returns for some time—in contrast to some of the nonagricultural activities that are more likely to yield immediate returns. On the other hand, for those households that are coffee producers, it would appear that RPS is serving as a risk-coping mechanism during the crisis, allowing them to maintain their coffee land and trees. Overall, RPS appears to be playing an important part in the risk-coping strategies of households.

Appendix

Table 12—Selected means and standard deviations by year, treatment group, and residence in coffee or noncoffee growing area

		RPS area		Control group	
	Year	**Coffee**	**Noncoffee**	**Coffee**	**Noncoffee**
Ln per capita real annual expenditures	2000	8.052	8.134	8.065	7.974
		(0.73)	(0.63)	(0.66)	(0.69)
	2001	8.190	8.212	7.762	7.875
		(0.56)	(0.54)	(0.66)	(0.65)
	2002	8.056	8.193	7.817	7.860
		(0.56)	(0.59)	(0.63)	(0.71)
Total hours worked last week	2000	78.9	80.3	76.4	87.7
		(58)	(56)	(62)	(58)
	2001	64.7	76.0	81.6	87.5
		(48)	(51)	(56)	(56)
	2002	74.2	77.0	96.9	85.9
		(47)	(55)	(63)	(59)
Average hours per worker worked last week	2000	23.1	24.3	22.5	25.2
		(10)	(9)	(12)	(9)
	2001	22.3	24.3	25.6	26.4
		(10)	(9)	(9)	(9)
	2002	24.0	24.2	27.7	25.0
		(9)	(9)	(8)	(9)
Percent 7–12 year old enrolled: girls	2000	57.8	87.1	70.6	88.7
	2001	93.0	95.5	74.5	88.6
	2002	93.5	97.0	78.5	90.3
Percent 7–12 year old enrolled: boys	2000	62.5	86.3	60.5	84.5
	2001	92.8	97.2	66.3	81.9
	2002	94.2	96.6	76.2	86.9
Percent households with coffee laborer	2000	26.1	2.3	27.4	2.7
	2001	13.8	1.8	23.1	1.0
	2002	13.6	0.5	19.1	0.8
Percent households with coffee producer	2000	5.9	0.3	7.3	0.2
	2001	3.7	0.3	3.1	0.0
	2002	4.4	0.0	4.1	0.0

Note: Standard deviations in parentheses.

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